

# Lawrence Livermore Laboratory

The Road to Fusion

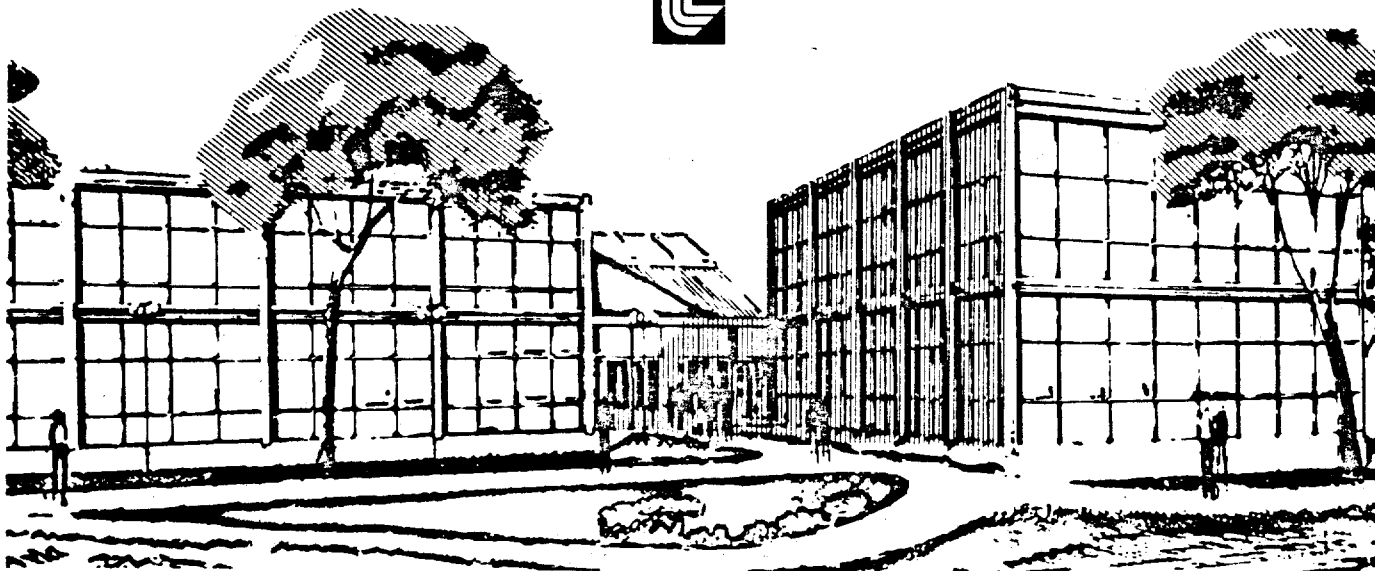
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## The Road to Fusion

R. F. Post

Someone once said "There is no such thing as a free lunch". Last night, when I was feeling both pleased and highly honored to receive the Maxwell Prize, I might have thought so. But this morning, when I have to face up to giving this talk, I know that the free lunch is a figment of the imagination.

There is one aspect about giving a talk like this that I do like, however. It is clearly a one-shot deal. I don't have to worry about my having to give a repeat performance - nor for that matter will you. Actually, that isn't quite true, I guess, because later on in this talk I am going to be borrowing heavily on another talk, one I gave in September at the International Plasma School in Erice, Sicily. The rather presumptuous title of that one, which I delivered in the evening before dinner on the day before the opening sessions was: "The Philosophy of Fusion Research". One comment that sticks in my mind from that occasion was the one made by a British participant, who shall remain nameless. Just as I was getting up to give the talk he leaned over and said: "I say, old chap, do hurry it up; I'm getting hungry". So much for philosophy.

But I really do want to talk this morning about what for lack of a better name I will call "The Road to Fusion" (not to be confused with the Bob Hope/Bing Crosby movies that a few of

us 39 year-olds remember). Perhaps because this is a one-shot occasion I feel a little freer to discuss such a topic than I would otherwise.

As it happens, I have spent a large part of my career as a physicist - that is 26 years of it - working on fusion research. For my own peace of mind, if for no other reason, I should be trying to understand the reasons that have kept me working in the field for so many years. Is fusion research, and its goal - fusion power - a sufficiently worthwhile pursuit that a physicist could spend his whole professional career working on it and feel good about it? I thought so 26 years ago, and I still feel the same way today, even though the practical realization of fusion power still lies many years in the future.

What are my reasons then? It is almost a platitude today to say that fusion research seeks a permanent solution to man's need for energy in the future. But in today's world of still relatively abundant energy we tend to forget that the quest for sources of energy goes back as far in time as man has been on this planet. The Prometheus myth is a symbol of this quest, and the motivations for it run very deep. They are: To seek freedom from slavery to the hoe, the shovel and the axe, and freedom from the threat of death by freezing in dimly-remembered ice ages. It is not an evil that we in this country have achieved that freedom. I will not argue with those who claim that we may have overshot the mark and that we could make

do with perhaps as little as one-half of our present per capita energy demands. But what are those energy requirements that go with our freedom? Counting all forms of energy they amount today to about 100,000 kilowatt hours per year per person. That figure is more than 300 times the amount of energy that could be extracted from a galley slave driven to the point of exhaustion every single day of the year. In other words, to feed and to maintain each one of us today requires an energy input more than two orders of magnitude greater than that which we could deliver with our own muscles even under the most extreme circumstances.

I don't want to overdramatize the point, but I am simply trying to put into perspective the fact that solving the problem of fusion would mean that, whatever else, man would have once and for all achieved the means to make himself free from a slavery to his needs for energy.

Please don't misunderstand me. Just as I believe that there are "no free lunches" in the broader sense, I also believe that there are no panaceas. The availability of a limitless energy resource such as fusion would of course not of itself solve man's problems. But it would be a solution - and I believe the best solution - to a problem, which if it is not solved in a timely manner will make the solving of man's other pressing problems far more difficult - if not downright impossible. Isaac Asimov has written a grim little story which paints a picture of what could happen in a formerly civilized

nation undergoing slow strangulation as its energy sources run out. While some native cultures still survive today where the per capita energy budget is much smaller than ours - that is all they are able to do - barely survive. I wouldn't wish that kind of a life on my great grandchildren - or anybody elses.

I cannot therefore agree with those that see modern large-scale energy technology as an evil that must be erradicated. These people should reread Edwin Markham's poem "The Man With the Hoe", and then see how they feel about the subject of energy.

All of what I have been saying up to this point is by way of explaining some of the reasons why I feel so strongly that the search for fusion power is an extremely important task. Not because fusion represents a panacea, but because fusion represents one member of an extremely limited class of inexhaustible energy sources. Furthermore, I believe that fusion stands alone as an energy source that can satisfy the multi-dimensional requirements of global safety, minimal adverse environmental impact, and universal availability of fuel, that should be the hallmark of any energy source for the future.

If you are willing to agree with me that achieving fusion is an important goal, and if you are at the same time optimistic - as I am - that the problems still standing in the way of achieving fusion will be solved, then I am sure that you will also agree with me that it is eminently worthwhile to

think very hard at this particular juncture about how best to achieve practical fusion power.

Some people may be discouraged by the fact that more than 25 years of research on fusion has not yet resulted in a demonstration of fusion power. I am not. I am encouraged by the long list of solid accomplishments in the physics and technology of fusion that we can point to and that undergird our present research efforts. I believe that we can learn from this past history of fusion - not only in terms of the specific scientific and technological accomplishments that have been made, but also in terms of guidelines for shaping a worldwide research strategy that will optimize our efforts to achieve fusion.

In trying to arrive at an optimum research strategy we can also learn from our critics as well as our supporters. I believe that we must take seriously both of the concerns of environmentalists who on the one hand wish to insure that fueling our energy sources for the future will make as little as possible negative impact on the environment and on the other hand insure that these energy sources will involve as little hazard of all kinds in their use as possible. In this connection, as I mentioned earlier, it is my belief that if we in fusion research do our job properly, fusion power can develop into one of the most environmentally benign and least hazardous sources of energy that man has ever employed.

I also think that we must listen carefully to those whose

business is the generation of electric power when they point out the need for reliability and for acceptable first costs for fusion power plants. We will certainly greatly delay the acceptance of fusion as a source of energy if fusion power plants are so big and expensive and hard to maintain that only a few electrical power systems could afford them - or use them.

With these things in mind - that is the lessons of history and the admonitions of our critics - I would like now to outline some elements of a world fusion research strategy that I feel will be important for the fusion research effort to be guided by for several years to come. I realize that many of these points have been emphasized by others, and that some of the items reflect personal biases of my own. I will leave it to you to agree or to disagree, but I ask you as scientists and as citizens to seriously consider the issues involved.

After I have listed these policy elements I would then like to spend my remaining time amplifying some of the points.

As I see it there are five policy elements that need to be implemented as we proceed toward the goal of fusion power. The first one of these is that we must

- Maintain a broadly-based program, one that includes both a spectrum of different approaches to fusion and a concern for fundamental plasma physics issues.

The next point is that we should

- Promote the development of fusion-relevant technologies, for example high intensity particle

beams and high field magnets. We need to do this both to speed the pace of the research and to undergird future engineering requirements.

The third point is closely related to the first point, but it is important in its own right. We need to

- Insure that there exists a proper balance between the research effort expended on "conservative" approaches, such as the tokamak, and that expended on more speculative approaches, for example the Field Reversed Mirror. As we attempt to maintain this balance we need to take seriously the lesson of history that "fads" come and go in fusion research, just as they do in other pursuits.

Fourthly, we should

- Encourage a search for innovative approaches, particularly those that may lead to simpler or more compact fusion power systems. At the same time, we should use the already considerable body of knowledge concerning plasma physics and plasma engineering to screen out the clearly unworkable ideas from the promising ones in assessing these innovative approaches.

Finally, and I believe that this is a point to which we need to pay increasingly more attention, we in fusion research need to

- Maintain a constant dialogue with other sectors of the

scientific community, with industry and with the public and its political representatives so that information and constructive criticism can flow freely in both directions and so that a broad political/economic constituency for fusion becomes established.

So my points are, in brief, that we need

A broad program, balanced as between conservative and speculative approaches and including the vigorous development of relevant technology. We need to stimulate innovation in search of simpler and smaller systems. And we need to maintain constant dialogue with the industrial sector, the electric power sector, and the public and its political representatives.

I would like now to come back for a moment to the matter of the lessons we can learn from fusion research's history.

One lesson that we can learn is that Darwin's Theory of Evolution seems to apply to fusion approaches! Compared to the number of species now being actively pursued the number of once flourishing and now extinct or nearly extinct species is quite large. To name a few: radio frequency confinement, the simple toroidal pinch, the linear theta pinch, the picket fence and/or simple cusp geometry, the Ixion rotating plasma and the toroidal octapole. Generally speaking the law of "the survival of the fittest" seems to operate in fusion research, too. In

fact it sometimes seems to work too well when some fragile new flower of an idea gets trampled underfoot in the stampede toward the latest bull in the pasture. Perhaps there is a two-fold lesson here: To be worth pursuing today a fusion approach must have sufficient hardiness and credibility to justify its support. But at the same time it does not have to be a close relative of the front runner or to have a 20 year history to be worthy of consideration. The Tandem Mirror idea bears no resemblance to a tokamak and there presently exists little experimental data bearing directly on the Tandem idea, but I think that it is nevertheless eminently worth supporting.

While we are talking about what the past can teach us, or how we can benefit from history's lessons, I would like to mention another, non-Darwinian, aspect of fusion that I think is very important. You might call it the phenomenon of the re-emergence of formerly extinct species. There are by now several examples of this effect. For example, John Dawson and others at UCLA are taking another look at toroidal multipole systems for their possible use with advanced fuel cycles - that is D-Helium-3 and other cycles producing lower neutron fluxes than the D-T reaction. Another example is the Reversed Field Z-pinch. The Zeta device in the U.K., although now extinct, provided data on the spontaneous emergence of field reversal in the toroidal pinch that has now been put on a solid theoretical footing by Brian Taylor in his elegant treatises on that subject. This has lead to a resurgence of experimental work on

reversed field Z-pinches.

Also, the Reversed Field Theta Pinch, which was looked at many years ago and then discarded is now reemerging as a viable technique for the investigation of the equilibrium and stability of field-reversed plasma entities. At Livermore we are again taking field-reversal very seriously, in connection with our ideas for the neutral-beam-driven Field Reversed Mirror, after a hiatus of several years since the work on the ASTRON was terminated.

So, new experimental data, new ideas, or new theoretical input can propel an old and nearly extinct fusion approach back into the limelight. But there is also another way that this can happen. This way is through technological advances. We worked for many years at Livermore, in the ALICE and Baseball I and Baseball II experiments in trying to apply the neutral beam injection technique to the problem of building up and maintaining a hot plasma in mirror systems. Although we learned a great deal about the anatomy of microinstabilities in low density mirror plasmas, our best efforts only yielded plasmas at densities that were 4 to 5 orders of magnitude below those that we needed to reach in order to be interesting from a practical standpoint. But then we, that is Lawrence Berkeley and Lawrence Livermore labs, developed the first high current neutral beam source modules and then stacked 12 of them up in 2XIIB to raise the neutral beam equivalent current to 500 plus amperes - more than three orders of magnitude higher than what

was available in the Baseball experiment. That made all the difference. What had been a major problem - that is plasma startup - turned out to be easy. And when we had achieved a high plasma density we were then able to examine the relevant microinstability problems, and found that the really important mode could be stabilized.

In a somewhat similar way the recent successes in achieving high ion temperatures in PLT have been a direct result of the availability of neutral beam injectors of sufficiently high power to surpass the limitations formerly imposed by ohmic heating.

The lesson to be learned here is that technological advances - whether by quantitative improvements in an older technique, or by the emergence of a new one (high intensity relativistic electron beams for example) - can transform previously apparently unworkable ideas into promising approaches.

But putting a new shine on an old idea is not the only thing that new technology can do: It can provide the means for implementing brand new ideas. It was the combination of the mirror physics learned in 2XIIB, plus the assured availability of high power neutral beam sources that provided the propitious environment within which the Tandem Mirror idea of Ken Fowler and Grant Logan sprang up at Livermore.

These remarks are by way of emphasizing the second point in my list of policy elements for a fusion research strategy -

namely that we should actively promote the development of fusion-relevant technologies. These technologies are in many cases the pacing elements in the rate of progress - particularly when it comes to major leaps in performance - such as we have seen in the mirror program, and recently in the tokamak program.

While we are still talking about the lessons of history let me comment on my first policy element - the importance of maintaining a broadly-based program. If we think back on some of the approaches that were once very much in vogue and are now extinct we can see what might have transpired if, at the time a particular approach was in vogue it had been singled out as "the chosen path" and all other approaches had been shelved. We would indeed have been in big trouble today. The gist of the argument for narrowing down is very familiar to you, I am sure. It goes something like this: "If you will just concentrate your energies single-mindedly on one approach you will not only more clearly define just what it is you have to accomplish, but you will at the same time gain the approval of those outside the program who have been waiting for you to make up your mind and tell them the one true way to fusion power." I claim that such arguments are both simplistic and dangerously fallacious, not only now but probably for the foreseeable future. Fortunately, in the U.S. and I believe in other countries as well, the need for a broad-based effort to achieve fusion is recognized. Most recently, in the U.S. we have had

the benefit of the report of the prestigious ad hoc group of scientists and engineers convened to review fusion research under the chairmanship of John Foster. They came out squarely for the importance of maintaining a broad-based approach to fusion. To quote their report:

"The strategy which we recommend for the next several years is to pursue fusion on a broad front: broad in the sense of vigorous support to several different conceptual physics approaches, and broad in the sense of an intensive physics/engineering analysis, tradeoffs and experiments to identify and resolve problems which could stand in the way of a practical fusion reactor."

I believe that there are many reasons why it is important to maintain breadth. Not only do we not yet know enough about the plasma state to feel cocksure about it, but also we have to clearly recognize the possible differences between devices that may produce scientific successes in the course of the research and those systems that can be finally successful from an engineering and an economical standpoint. The best magnetic confinement system might turn out to be the least desirable system from an economic standpoint. It might turn out in fusion as it says in the Bible---"And the last shall be first". Let's face it; we are still learning our way around in fusion, despite our many successes.

Next, I would like to amplify on my fourth point, that we

should encourage a continued search for innovative approaches. This has always been an important element in fusion research, to our credit, but I think its importance has increased not decreased with time. Anyone who has worked on fusion, and who has fully understood the significance for the future of man of achieving fusion power, has also felt the urge to find some simpler, or more compact, or more elegant approach to fusion. I know that it is not possible to innovate on command, but at least the climate should be favorable for it when it does happen. What is also the case now, that was also not true 10 or 20 years ago, is that we have a well stocked cupboard of tools - both theoretical and experimental - with which to evaluate the workability of a new idea. So long as we use these tools in a constructive way, and not merely punitively (as in the NIH syndrome - Not Invented Here), then these tools can and should be used to sharpen up our judgment in the process of innovation.

In connection with the objective of looking for innovative approaches, there is an aspect of fusion as a source of energy that I have not mentioned yet, but which I believe sets it apart from virtually all other sources of energy that man has ever employed. This aspect concerns the tremendous span of physical conditions and the large number of potential fuel cycle options that can be contemplated, particularly when we consider fusion power in the light of a long term energy source that will be subject to improvement and perfection for many

years to come. Although the deuterium-tritium cycle is almost surely going to be the first one used, I have a feeling that D-D or D-D-Helium-3 will not be far behind. Even the possible synergism with fission that the hybrid represents is an option that might be valuably employed, at least during an interim period.

But even if we for the present ignore alternative fuel cycles there exists the great range of fusion plasma conditions that are potentially available to the fusion innovator or the fusion engineer. The fact that fusion is a binary reaction means that in magnetic fusion the fusion power densities vary from watts per liter to gigawatts per liter of reacting plasma over the presently conceivable range of beta values and confining field intensities. In pellet fusion the corresponding power density figures are of course astronomical. Similarly, in magnetic fusion the confinement times required to achieve net power range from 10's of seconds to microseconds. To my mind there is no parallel to the overall options and domains available for fusion power as compared to any other source of energy - fission included. While we must always not ignore practical constraints, we need also always to keep in mind the breadth of possibilities that are inherent to the fusion process as we seek to find the best and shortest path to fusion power.

Finally, I'd like to say a word or two about my fifth point - maintaining dialogue with those outside the fusion community

and building a constituency for fusion. We should certainly know by now that there are no easy solutions to fusion power. I'm sure that God planned it that way - in accordance with the general cosmic principle that nothing that is truly worthwhile is attained without working hard for it. But I hope that it is also now clear to most of us in fusion research that the goal of fusion power will be attained, provided we really want to attain it. Where the problem comes is with those that either don't understand what we are trying to accomplish, or who are by nature suspicious and pessimistic about anything new and untried. As well as those who are cheering us on, the road to fusion is lined with pessimists, doubters and cynics who say "Can't work", "Won't work", or "Who wants it?". There are even those, the ones that I referred to earlier, who are sincerely unhappy with our successes, since they realize that the achievement of fusion would indeed represent a solution to the problem of growing energy utilization and that fact would therefore make us, the people, less willing to accept the so-called "soft technologies" - a windmill on every roof and all of us riding bicycles, handmade ones of course. While all of us recognize the nostalgic appeal of the soft technology scenarios, I cannot accept their assumptions as I understand them concerning the elimination of large-scale energy systems.

We cannot however solve the political problems represented by the views of our detractors by ignoring them. I contend that we in fusion research must increasingly carry out

constructive dialogue with all who are outside the fusion community, through public talks, through writing for the popular audience, and through contact with our political representatives. Above all, we need to "tell it like it is" with respect to what we have achieved in our researches - and what we have yet to achieve. We are dealing with far too important an issue to play fast and loose with the facts.

I believe that we, in the last decades of the 20th century, have a rare privilege to be the ones that set man free from the sad consequences of decreasingly abundant energy. With this privilege comes the responsibility of seeing to it that the task is accomplished in the best way we know how. By "in the best way we know how" I do not mean fusion visualized in the narrow sense of a scientific/technological turkey shoot where the object of the game is to be the first ones to score a bullseye, thereby showing once more how clever we are. Rather than that I mean fusion seen as the gift that God intended man to have, the realization of which should be a happy task - a task carried out responsibly by all of those who are involved, working toward a common goal.

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